ENHANCING WILD SALMONID RECOVERY IN THE SACRAMENTO − SAN JOAQUIN RIVERS AND DELTA−BAY SYSTEM: A MULTIDISCIPLINARY APPROACH LINKING NOVEL REMOTE SENSING TOOLS AND STANDARDIZED CROSS−SITE COMPARISONS TO ASSESS RESTORATION POTENTIAL OF FRESHWATER HABITAT

Mark S Lorang

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0243: ENHANCING WILD SALMONID RECOVERY IN THE SACRAMENTO − SAN JOAQUIN RIVERS AND DELTA−BAY SYSTEM: A MULTIDISCIPLINARY APPROACH LINKING NOVEL REMOTE SENSING TOOLS AND STANDARDIZED CROSS−SITE COMPARISONS TO ASSESS RESTORATION POTENTIAL OF FRESHWATER HABITAT

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This proposal lacks connections within the scientific community and restoration programs of the Sacramento−San Joaquin and Bay region. There is no evidence of trial efforts to demonstrate effectiveness of the approaches suggested by this project. This is a “grand scale” project with no evidence of success other than in the NW−Pacific programs. It also is leaning on reputation of PIs to carry the project and assumes success elsewhere. The proposal has many wonderful objectives with little evidence of potential application to Bay Delta region. There is no evidence of any significant data mining from within the Bay Delta region to demonstrate the applicability of the approach developed in NW.

Additional Comments:

This proposal lacks connections within the scientific community and restoration programs of the Sacramento−San Joaquin and Bay region. There is no evidence of trial efforts to demonstrate effectiveness of the approaches suggested by

this project. This is a “grand scale” project with no evidence of success other than in the NW−Pacific programs. It also is leaning on reputation of PIs to carry the project and assumes success elsewhere. The proposal has many wonderful objectives with little evidence of potential application to Bay Delta region. There is no evidence of any significant data mining from within the Bay Delta region to demonstrate the applicability of the approach developed in NW.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The technical reviewers differed considerably in their evaluation of the proposal; the primary and secondary panelists reviewing the proposal largely agreed with the more critical technical review. The panel had significant concerns regarding the likelihood of the project’s success because the proposal does not demonstrate existing connections to researchers in California and on−going related projects in the Bay−Delta region. In addition, this is a large−scale project that has not been preceded by a pilot in the region, and without demonstrated success in other regions comparable to the Bay−Delta region. Furthermore, the Shifting Habitat Mosaic model, which has been developed from relatively unmodified or wild river systems, may not be an appropriate model for the highly altered Bay−Delta system, because these rivers are highly constrained by man−made structures. This was considered a technical deficiency.

Technical Review #1

proposal title: ENHANCING WILD SALMONID RECOVERY IN THE SACRAMENTO − SAN JOAQUIN RIVERS AND DELTA−BAY SYSTEM: A MULTIDISCIPLINARY APPROACH LINKING NOVEL REMOTE SENSING TOOLS AND STANDARDIZED CROSS−SITE COMPARISONS TO ASSESS RESTORATION POTENTIAL OF FRESHWATER HABITAT

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**I get worried when I see goals like " Enhance a broader perspective and understanding…".** **Those are some pretty fuzzy objectives. This project really smells like a boon−doggle in the making, with funding to various parties to stand knowingly on the riverbank expounding on their various theories about how returning the river to some natural state will improve salmon productivity.**
Rating	**poor**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**As far as I can tell the justification for this project is to fly researchers around to some nice river systems and to collect lot's of physical information that won't be much use to CALFED objectives. I can't see much justification from CALFED's perspective.**

Rating	**poor**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach of the proposal, and of SARON for that matter, is deeply flawed. I doubt whether the effects of "human activities are predictable and therefore modellable in the SHM context" (sic) or in any context. Restoration activities need to be assessed in real−world experiments via adaptive management based on sound experimental design, good monitoring, and meaningful treatments. Rather than study the key population dynamics such as smolt output, early life stage survival, growth, etc. and relate those to management or physical habitat, the proponents are going to measure and model physical characteristics alone. The weaknesses of this approach are well established. The river restoration workshops sound pleasant but I don’t see much coming out of them.**
Rating	**poor**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is fully documented but is not sound, and therefore has no likelihood of success.**
Rating	**poor**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The mindless synthesis of restoration actions and physical habitat will be of little to no use.**
Rating	**poor**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**No products of value will come from this project. I doubt the databaes or GIS layers will be of any use to anyone.**
Rating	**poor**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Dr. Stanford is a leading authority on river ecology. He could probably contribute to many CALFED processes, but not in the context of the proposed work.**
Rating	**poor**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is very large and useful outcomes are extremely unlikely.**
Rating	**poor**

Overall

Provide a brief explanation of your summary rating.

Comments	**I have never seen a proposl for this much money that is based on such shaky objectives and flawed scientific approaches.** **THIS PROJECT SHOULD NOT BE FUNDED.**
Rating	**poor**

Technical Review #2

proposal title: ENHANCING WILD SALMONID RECOVERY IN THE SACRAMENTO − SAN JOAQUIN RIVERS AND DELTA−BAY SYSTEM: A MULTIDISCIPLINARY APPROACH LINKING NOVEL REMOTE SENSING TOOLS AND STANDARDIZED CROSS−SITE COMPARISONS TO ASSESS RESTORATION POTENTIAL OF FRESHWATER HABITAT

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This is an ambitious, generally well conceived and intriguing proposal aimed at evaluating salmonid habitats at the landscape scale and using comparisons with relatively intact systems as a template for restoration in the CALFED drainage. The integration of database management, multispectral imagery,and regional workshops to facilitate communication and data sharing would certainly provide a unique perspective for new ways to approach salmonid recovery, a perspective that is needed since arguably existing schemes have not been successful. This proposal may offer a new approach, though with a big price tag!**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The SHM approach is interesting but supporting evidence for its critical role in salmonid**

	production I didn't find all that convincing, particularly with regard to wild salmonid recovery (goal 3) based on this model. Again, very interesting and intriguing hypothesis, but seems that more data are needed to test this hypothesis prior to basing mgmt on it. The SHM model undoubtedly identifies hydraulically complex reaches, but no supporting evidence was presented to support the contention that these complex areas are in fact critical to overall production. The inference is there sure, but not hard data presented in the proposal. Showing that a SHM reach supports x% of the total salmonid production in a drainage would lend needed support to this contention, a contention on which their main hypothesis hinges. In short, identifying these reaches as 'critical' needs substantiation rather than apparently assuming they are critical because they are complex and perhaps rare in the drainage. For this reason, my suggestion is that an initial pilot project illustrating how data would be collected on a sample reach or reaches, particularly in relation to salmonid habitat use, would be a good first step prior to full funding as a full−scale implementation project.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**I think the integration of the quite wide ranging communication and research objectives is a grand idea. The authors appear to have a good deal of experience thinking and planning such large scale, ambitious,**

	cutting edge type projects. I found all the objectives well crafted with the exception of objective 3. I found the wording "document habitat and process−specific salmonid..productivity" ..."focus on habitat requirements ..in relation to population structure" ..."biotope specific productivity" to be quite vague. In fact, I found the integration of spectral imagery with field ground truthing of salmonid habitat use/productivity to be the least explicit part of the proposal.In contrast, the other objectives I found to be much more clearly presented and hence more likely to be attainable.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As noted, with the exception of objective 3, I found the rest of the approach well documented and feasible with a high likelihood of success based on their similar work in other river systems within the SARON system. The scale of the project is so large, that it makes it risky in and of itself, but again, I believe that is part of the attraction of it, of viewing salmonid habitat and restoration from a newer, larger scale, more integrated and comprehensive approach.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**It is unclear how the database management will handle existing information. Although having all this disparate information on aerial**

	photos, fish data, management information, all in one place is attractive, it was unclear how large a task this might be, and in reality how useful. An example illustrating the efficacy of this approach would have been helpful.
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The multispectral images alone I'm sure will generate a great deal of interest. The illustrated examples of river reaches are intriguing and give a unique point of view of the system. As far as the white paper and mgmt implications, I get the sense that authors believe that managing SHM's is key to protection in intact systems and to recovery in damaged salmonid ecosystems. But again, while certainly an intriguing hypothesis, I believe more data are needed to test this contention rather to proceed as if it is true, at this point. Certainly the different systems in the SARON network should be able to test this hypothesis across a wide variety of different systems. The workshop benefits, like most education, will be difficult to gauge the benefits. I truly like the idea of having biologists view many different systems, as myopia easily develops if one's experience is limited to certain types of systems, particularly disturbed ones, which can after a while, take on the normative baseline in one's mind.So an intriguing idea to incorporate this front and center as an objective rather than list it as we usually do, as an afterthought.**
Rating	**excellent**

Additional Comments

Comments	**Proposal was well crafted and well written for such a complex study comprised of several rather disparate parts.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**seems to be quite good with respect to these very difficult and complex types of projects. The only question mark in my mind, as mentioned, is the integration of actual salmonid habitat use in the field with the aerial photos, this did not seem to be fleshed out as well as other objectives.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Very ambitious project and thus high cost. The intricacies of what the authors are trying to do with imagery, data base mgmt, field collections, workshops, entails a lot of players and a lot of costs. I am not well enough versed in the imagery portion of the project to comment on that (quite significant) cost, but the rest of it seemed reasonable to achieve the broad reaching objectives of the study.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**There's a lot of intriguing, thought−provoking aspects of this project that are attractive and could lead to significant new approaches to how we view salmonid habitat productivity as a template for new management directions. It is high risk and high cost, so rather than vote all or nothing, my suggestion would be if considered too expensive or risky, that at least a pilot imagery, initial field collections, and initial workshop phase be funded as a precursor to full scale funding. In short, the study is unique enough and has enough potential that some level of funding would be recommended as a minimum to assess full capabilities of their proposed approach.**
Rating	**excellent**

Technical Review #3

proposal title: ENHANCING WILD SALMONID RECOVERY IN THE SACRAMENTO − SAN JOAQUIN RIVERS AND DELTA−BAY SYSTEM: A MULTIDISCIPLINARY APPROACH LINKING NOVEL REMOTE SENSING TOOLS AND STANDARDIZED CROSS−SITE COMPARISONS TO ASSESS RESTORATION POTENTIAL OF FRESHWATER HABITAT

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The topic of this project are related to the CALFED goals of understanding ecological process and their relationship to water management and key species. By relating the function of pristine systems to the more impaired function of the Sacramento and San Joaquin Valleys and Bay Delta system (SSJV−BD), the authors hope to assess restoration potential of the SSJV−BD.** **Their goals and hypotheses are clearly stated and their purposes clear. Linking the SSJV−BD system to the large Salmonid Rivers Observatory Network (SaRON) will have clear benefits in terms of understanding the function of systems within SSJV−BD and contrasting them with fully functioning or recovering systems. It puts SSJV−BD in a larger context and allows for a greater range of data sharing and coordination among systems and scientists.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Is the study justified relative to existing knowledge?** **Yes** **Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work?** **Yes, the SHM is a well−developed concept, though difficult to test mechanistically.** **Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?** **SaRON gives this project the larger context and tests ideas that make a pilot or demonstration project redundant; therefore, if this project is funded, I recommend full−scale implementation.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**One of the goals is to develop a consortium white paper that has specific recommendations and action protocols for SSJV−BD developed from cross−site comparison with the SaRON project. Although the linkages will be made, how those linkages will be directly relevant for specific, strategic management recommendations remains unclear. Although they propose to describe the physical environment of this system in detail, they do not intend to describe the political and water use components, eliminating the possibility for strategic assignment of restoration areas. They assume a great deal is already known about the**

	anadramous salmonids in this system, and I do not know if this is a valid assumption. They also propose to test the Shifting−Habitat−Mosaic (SHM) but do not state specific predictions, only general predictions, regarding SSJV−BD if SHM applies. **The approach is not novel, the methods established, and the conceptual foundation is in place, which makes the project more feasible, but is unlikely to result in new breakthroughs regarding specific restoration actions with positive results.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Is the approach fully documented and technically feasible?** **It appears so, though the authors "assume that a great deal is already known of the basic biology, population density, growth rates, and genetic structure of anadramous salmonids in the SSJV−BD system because these are generally some of the first studies conducted within an ESA framework." Because the system is so large, I am not sure this is a really safe assumption.** **What is the likelihood of success?** **I believe that the authors will be able to disseminate their ideas and conceptual framework and apply it to SSJV−BD, but I am not sure that specific recommendations and action protocols will prove useful or feasible.** **Is the scale of the project consistent with the objectives and within the grasp of authors?**

	Yes.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Data from this study will be put into a larger database encompassing many systems. That should prove useful for examining the transferability of models and concepts from the SSJV−BD to other systems that are either pristine or undergoing restoration. This allows for a 'space for time' comparison and projections of the potential outcomes of a variety of restoration activities.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	I think that ecological understanding of SSJV−BD will improve based on this project, and that remote sensing data will prove useful for a variety of restoration projects. Inclusion of SSJV−BD into the larger SaRON data set should give managers and scientists a larger context in which to do their work. I am not certain that the proposed white paper will result in specific management recommendations that will be both strategic and likely to produce good results; however, by placing the system in a larger context, the product will greatly inform resotoration activities in the region, which should prove invaluable to keep

	managers from ‘reinventing the wheel’ in their restoration activities and ecological questions. This is undoubtedly a very valuable product from this study.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors are well known, respected, and absolutely capable of seeing this project through and are part of a larger infrastructure that can easily take in the SSJV−BD data and concepts. Part of the budget, however, provides for logistics and facilities for the postdoctoral researcher doing on the ground work in SSJV−BD. This will be essential because on−the−site facilities are not presently available to the authors.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	This project is huge in scope, has high personnel costs, requires some funding for infrastructure and equipment, and is therefore very expensive. The River Restoration Workshop the authors propose and visits to SaRON observation rivers is a costly endeavor, but

	should help accomplish the transfer of knowledge among systems and scientists; however, I would reduce the scope of the initial trip to 'reference' sites to one or two locations (Including the Flathead Biological Station) to reduce costs.
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**In essence, this is a very well written and exciting proposal and has all of the components of a grand project; however, it is unclear if this study will simply continue the "conceptual foundation" that gives a theoretical basis for thinking about these systems or result in specific, strategic recommendations that result in real, feasible, and productive on−the−ground management for SSJV−BD. Recommendations like, "re−establishing natural processes such as enough flooding to initiate cut and fill alluviation" are unclear, and the spatial allocation of these restoration endeavors is most strategic when put in a political and multiple−use context. It is also unclear if spatial analysis of salmonid populations is available to put within this context. However, by doing cross−site comparisons, they will be able to test the universality of the SHM concept and advance ecological knowledge in general concerning salmonid ecology and functioning of salmonid ecosystems, all of which will inform, if not direct, management decisions in SSJV−BD.** **Data sharing and data management activities of this project should prove invaluable for understanding the SSJV−BD system, and the combination of data from SSJV−BD with data from other systems in SaRON will put its functioning in a larger context and help managers establish conceptual benchmarks for restoration goals. I see the SHM concept becoming a conceptual paradigm**

	like ‘Natural Flow Regime’ and the ‘River Continuum’ or the ‘Network Dynamics Hypothesis’ – the authors do not really propose to test this conceptual paradigm or contrast it with others, but rather apply it to the SSJV–BD system. This may be helpful for informing restoration and putting the system in a larger context, but does not further us along in finding a ‘universal’ concept for salmonid restoration, if such will ever exist. **Despite my pragmatic criticism of the SHM framework and its applicability to decision making, I think this project is worthwhile. Panelists should carefully weigh the benefits of the conceptual outcomes, data sharing, educational outreach, and the larger context that will result from this project against the costs associated with its large scope. Because SaRON and its associated programs are well established and the data management and informatics system tested and important for worldwide coordination of salmonid recovery, I believe that setting SSJV–BD within this larger context will be worth the costs.**
Rating	**very good**